August 17, 1976

Dr. Eugene Garfield
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Dear Gene,

Thank you for sending me the article by Rustum Roy in the Journal of Metals concerning citation study of departments of material science.

I am glad to see that some work is getting under way on the empirical validation of relative citation counting. It seems to me that everyone of Roy's criticisms is justified and deserves to be taken into account in using citation material. I am not even sure that raw citation counts even do very well as a zero order index unless one has some means of weeding out the "anomalies" of papers on methods, not to mention the other sources of noise that are given by Roy.

For example, I was intrigued to see your recent report of frequently cited old papers, but I would ask you yourself to go over that list and try to reach some conclusion as to how well it reflects the profile of contemporary interest in the work that was done in that period. My own examination suggests that obliteration occurs in a rather haphazard and sporadic way across various fields and that there is a certain tradition about continuing to cite certain old papers precisely because they have been cited before. On the other hand, when I can see a review appearing in last month's Bacteriological Reviews on Neurospora biochemical genetics and this makes no reference at all to the pioneer paper by Beadle and Tatum, you can plainly see obliteration at work in a way that would confuse anyone who was trying to discover the pattern of influence of Beadle on the development of the field!

I accept and understand that one might be able to discount the whole set of anomalies by making carefully controlled comparisons of large aggregate groups in which these idiosyncracies might be averaged out but which are operating in a similar environment of publication. One problem is, we do not have any very good alternative measures to use as yardsticks for comparisons of impact established by the use of citation indexing.

Sincerely yours,

Joshua Lederberg
Professor of Genetics

JL/rr